inevitable result is a dismal failure. It is not quite clear for what class of reader the work is really intended, for in the preface we read that it "is intended to be a source of information and ready reference for the textile chemist," while on the title-page it would appear from the continuation of the main title, "being the syllabus of a lecture course adapted for use in textile laboratories," that it might be intended for some other purpose, possibly for teaching. In either case it has missed its mark.

These "knowledge in a nutshell" publications on technical chemistry, of which there appears to have been an increasing supply of late years, may be just the sort of thing that please people who like that sort of thing, but although there are a few exceptions, it is doubtful whether they do much good, while, on the other hand, they may do distinct harm through creating, under pretentious titles, a totally misleading impression of the subject as it presents itself in actual practice.

Naturwissenschaftliches Unterrichtswerk für höhere Mädchenschulen. By Prof. Dr. K. Smalian. Auf Grund der Bestimmungen vom 19 December, 1908, über die Neuordnung der höheren Mädchenschulwesens in Preuszen bearbeitet von K. Bernau. II Teil: Lehrstoff der VI Klasse. Pp. 80. Preis 1.80 marks. III Teil: Lehrstoff der V Klasse. Pp. 127. Preis 2.25 marks. (Leipzig: G. Freytag; Vienna: E. Tempsky, 1909–10.)

IN the early part of last year a notice appeared in NATURE of Dr. Smalian's "Leitfaden der Tierkunde für höhere Lehranstalten," a work comprising a zoological text-book in separate fasciculi intended for the use of the various classes in German high schools. The fasciculi now before us form part of another work designed on somewhat similar lines for the use of girls' schools, but including botany as well as zoology. The general commendation bestowed on the "Leitfaden" may be extended to the present text-book, with the addition that we have little fault to find with the coloured plates of animals, while those of plants are excellent examples of German colour-printing, and worthy of all praise. Each of the two fasciculi now before us is divided into a botanical and a zoological portion; and it may be presumed that the same holds good for the other portion of the series. The zoological section of the second fasciculus is devoted to vertebrates, and that of the third to arthropods. A number of well-known species of mammals are, however, described in the first fasciculus.

The general plan of the work is similar to that of the "Leitfaden," the various orders being treated in systematic order, and a certain number of typical species being selected for comparatively full notice, while other groups are treated more briefly. In the case of the species selected as types, leading features in the external form and structure and noticeable traits in the matter of habits are touched upon; and throughout the work technicalities are, so far as possible, avoided. The only scientific names introduced are those of species, ordinal and family groups being referred to by vernacular designations. In the main the species represented in the illustrations seem to be correctly named; but in one of the coloured plates the monkey designated *Cercopithecus sabaeus* is clearly *C. aethiops* or one of the allied forms, as it has the distinct white brow-band of the latter, which is absent in the former. So far as we can see, the book appears admirably suited for its purpose, although it by no means follows that it would be equally well adapted to the needs of English schools.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The Meaning of "Ionisation"

IN asking for precision of language (p. 487), Prof. Armstrong shows how much he has fallen behind his time. If he had kept abreast of the recent developments of the principles of science, he would know that to be precise means running the risk of being wrong, which is to be avoided at all costs. Prof. Armstrong evidently belongs to an antiquated school which believed that scientific discoveries are made by forming definite ideas of things, even though these cannot be seen and handled. That is a standpoint which is abandoned, and we have entered on a new era. Science now aims purely at obtaining an equation which, without committing itself to any definite views, gives the required relationship between the brain impressions taking place in that particular dimension of a many-dimensional complex, which we identify with time. I am sorry if in this statement I have committed myself to the existence of a brain—it was lapse due to a weak concession to the prejudices of my youth, and would have been impossible in a thorough-going adherer to the new faith.

But to come to the point. If Prof. Armstrong will bring the theory of entropy to bear on the principle of least resistance to a cheap appearance of sagacity, he will discover, not what Arrhenius meant by ionisation—that is unimportant—but what he ought to have meant and would have meant if he were a chemical physicist such as we make them now.
A. S.

The Fertilising Influence of Sunlight.

THE letter on the above subject by Mr. and Mrs. Howard in NATURE for February 17 raises a question of much scientific interest and of considerable importance in tropical agriculture. In some of the text-books it is stated that the hot sunshine of tropical or subtropical climates must injure the productiveness of the soil, since it kills bacteria. On the other hand, experiments on the partial sterilisation of soil by other means—such as heat or volatile antiseptics—shows that the killing of bacteria (as distinct from spores) leads to an increased, and not a diminished, productiveness. The apparent discrepancy is now cleared away, and we have Mr. and Mrs. Howard's authoritative statement that strong sunlight has beyond question a beneficial effect on productiveness.

There is a close resemblance between the effects they describe and those that have been obtained with partially sterilised soils by myself in conjunction with Dr. Darbishire and with Dr. Hutchinson; in all cases the effect is that of a dressing of nitrogenous manure. Dr. Hutchinson and I have traced this to an increased rate of decomposition of organic matter after partial sterilisation, and have shown that the increased activity is due to the destruction of some agent, probably large organisms, which had previously interfered with bacterial development. The question is, Could sunlight partially sterilise a soil and kill the large destructive and competing organisms that we suppose limit productiveness?

There are at least three factors involved: sunshine dries the soil, heats it to a certain temperature, and may have a direct chemical action fatal to the cell. We are at present studying the effect of dryness and of temperatures lower than 100° (at which we have previously worked), but the direct effect of sunshine is not easily investigated here. Some preliminary experiments I made at Wye during the summer of 1906 indicated that soil exposed to bright sunshine for a period of ten days subsequently absorbed oxygen more rapidly, *i.e.* showed a higher rate of bacterial activity, than another lot kept shielded from the light. The effect was comparable with that produced by volatile antiseptics, and, so far as the experiment goes, it shows that sunlight could, equally with these, remove the factor limiting productiveness in ordinary soils. I have several times attempted to extend the experiment, but

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have been unfortunate in timing it, and have missed the continuous spells of strong sunshine

We could not hope to attack the problem so successfully here as in India, and it would be interesting to have some Indian work on the subject. The experiments of Mr. and Mrs. Howard in conjunction with Mr. Leake on the "fertilising influence of sunlight" may be expected to throw a great deal of light on a very important problem.

E. J. Russell.

Rothamsted Experiment Station, Harpenden.

The Spectrum of Bacterial Luminosity.

In the course of some experiments on the phosphorescence of minerals, especially with regard to the emission of ultra-violet radiation, it was suggested to me by Prof. Strutt that it would be of interest to examine the spectrum of the light from luminous bacteria. Through the kindness of Sir James Dewar, Prof. Strutt was enabled to furnish me with a dish containing bacteria produced from fish which, when viewed in a darkened room, were seen to glow with a greenish-blue light bright enough to enable one to read a watch. The dish was placed in front of the slit of a quartz spectrograph of rather crude design, constructed with lenses of short focal length so as to utilise the light available to the fullest extent. A special rapid photographic plate was first exposed to the radiation from the bacteria for forty-six hours, and then to the light from a cadmium spark for one second.

The plate when developed exhibited strong evidence that the spectrum of the light emitted by the bacteria consisted of a continuous spectrum extending from wave-length 5000 (the lower limit of the sensitiveness of the plate) to wave-length 3500 tenth-metres, together with a well-marked bright band of wave-length 4000 approximately. Practically the same result has been obtained from bacteria produced from meat. The greater portion of the radiation is absorbed by a film of nitroso-dimethylaniline.

Mr. J. E. Barnard, in his article on luminous bacteria (Nature, April 10, 1902), states that the light emitted by these organisms is confined to a small portion of the visible spectrum, and never extends into the ultra-violet or infra-red. The photographs I have obtained indicate that in some cases these bacteria emit ultra-violet radiation, and I hope with another spectroscope, now in course of construction, to obtain more definite information.

R. W. Forsyth.

Royal College of Science, South Kensington, S.W.,
March 1.

Self-fertilisation and Loss of Vigour.

Some of your readers will remember that in the introduction to "Cross and Self-fertilisation of Plants," Darwin inserts a report by Sir Francis Galton on seven tables of measurements relating to the relative heights of the offspring of cross and self-fertilised plants. "It is a very remarkable coincidence," says Galton, "that in the seven kinds of plants the ratio between the heights of the crossed and the self-fertilised ranges in five cases within very narrow limits. In *lea mays* it is 100 to 84, and in the others it ranges between 100 to 76 and 100 to 86."

If Table A of Darwin's book be referred to, it will be found that these ratios recur in many more than five cases. I write to suggest—with great diffidence, as I am a mere amateur in such matters—that the persistence of these ratios is capable of simple explanation on the basis of the Mendelian theory of heredity.

The distribution or "array" of a Mendelian family, crossed at random, is given by the expansion of $(3D+R)^n$. where n = the number of pairs of characters involved, and the powers of D and R respectively represent the number of dominant and recessive somatic characters of each individual. If, now, each individual be self-fertilised, the array can be shown by simple algebra to become $(5D+3R)^n$. Similarly, in the next self-fertilised generation, the array becomes $(9D+7R)^n$, and so on. The average number of dominant characters in each generation is, consequently, $\frac{3}{4}n$; $\frac{5}{8}n$; $\frac{9}{16}n$, &c., which give the ratios 100 : 83 : 75, &c.

It is surely something more than a "remarkable coincidence" that these ratios agree in so many cases with the ratios observed by Darwin?

A. B. Bruce.

School of Agriculture, Cambridge, February 19.

Vision and Colour Vision.

With reference to the comment on my lecture at the Royal Society of Arts in Nature of February 17, I did not give the evidence in favour of the visual purple being the visual stimulus transformer on account of the time at my disposal. This evidence is very strong, and the facts are inexplicable on any other hypothesis. Many physiologists have tried to assign different functions to the rods and cones, but these theories have failed because all the functions which were said to be the exclusive property of the rods have been found, only gradually diminished, in the fovea, in which only cones are present. For instance, von Tschermak, Hering, Hess, Garten, and others, have found the Purkinje phenomenon, the variation in optical white equations by a state of light and dark adaptation, the colourless interval for spectral lights of increasing intensity, the varying phases of the after-image, in the fovea, only gradually diminished. The complete absence of any qualitative change between the foveal and extra-foveal regions is a very important fact in support of the hypothesis that the visual purple is the visual substance. There is also the fact, mentioned by Helmholtz, that a perceptible interval elapses before we see with the fovea, after the rest of the retina, when the eye has been previously some time in darkness. Hess has also pointed out that the recurrent image is present, but retarded at the yellow spot. All these observations and many others agree with my statement that the visual purple can be seen between but not in the cones of the fovea.

F. W. Edridge-Green.

Hendon, February 19.

The Methods of Bird Fanciers.

May I ask for information on the following matter?

Happening to be walking in a poor quarter of Acton (Chiswick) with a friend, we were astonished to see hanging outside a house, on either side of the door, two small bird-cages completely enveloped with dark cloths carefully fastened. The cages evidently contained birds, for from one of them came the song of a chaffinch.

Had the birds suffered the torture of having their eyes put out to make them sing, or is this a method resorted to instead of blinding them?

The caging of wild birds is so very cruel that it should be exposed in all its barbarous ways.

Some of the wild birds are kept in such small cages that there is scarcely room for them to turn round.

E. L.

It is customary for bird fanciers amongst the lower classes to match one chaffinch against another, often in public-houses, the bird which sings the largest number of notes being adjudged the victor. When the cages are carried about they are usually wrapped up in dark cloths, and it is said that one of the objects of keeping the birds in the dark is that they should imagine it is night while they are covered, and that on the cloth being removed, thinking it is day, they burst forth into song. As it is believed by some that birds sing better when they are blinded, it may be, as "E. L." suggests, that the covers were used in the case mentioned either to avoid practising the cruelty of putting out the birds' eyes or so that the fancier might not render himself liable to the punishment for doing so. More likely the birds that were kept covered were newly caught, and the object was to prevent them from dashing themselves against the bars of the cages in their endeavours to escape. As has often been pointed out, one reason for the smallness of the cages used is to lessen the danger of the birds killing themselves, as in a larger space they could begin to fly and would strike with greater force.

Wilfred Mark Webb.

Title of the Natural History Museum.

In Nature of February 24 (p. 489) Mr. Hobson suggests as both "suitable and adequate" that it should be called the "British Museum of Natural History."

Why not omit "British" and "of," re-arrange the order of the words, and call it "The Natural History Museum," adding "London" if necessary?

Torquay.

F. Howard Collins.

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